THE MOLARITY OF MOLECULAR THEORY AND THE MOLECULARITY OF MOLAR THEORY

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Dinsmoor (2001) rejects shock-frequency reduction as a reinforcer for avoidance behavior, and considers this to be an invalidation of so-called molar avoidance theory. This is a narrow view of operant avoidance theory, for which shock-frequency reduction is by no means the only reinforcer.

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The claim can be made that the so-called molecular and molar approaches are not dichotomous. Thus, Dinsmoor (2001) interprets Herrnstein–Hineline avoidance (1966) not in terms of the delay to shock of the feedback from the individual response but in terms of the longer *average* delay to shock of the effective response compared to *all other* responses. This is hardly completely molecular, and this sensitivity to relative average delay must be assumed to be effective, despite the fact that the effective response itself may often be immediately followed by shock because of the random distribution of shocks on both shock-frequency schedules. Is this sensitivity to relative average delay really simpler than assuming that the effective variable is shock-frequency reduction?

Furthermore, Bersh and Alloy (1978, 1980) pointed out that, in their procedure in which interresponse times less than or equal to a specified criterion reduced shock intensity or duration, no single response provided differential feedback with respect to shock aversiveness. The basis for reduced shock aversiveness was the interresponse time, by definition a relation between successive responses. This is again not entirely molecular. To preserve a somewhat molecular account, Dinsmoor (2001) must assume that the safety signal depends upon the animal's sensitivity to the time elapsing since the feedback from one response to the feedback from the next response. Moreover, a pair of responses that met the interresponse-time criterion did not guarantee exposure to less aversive shock. Because shock occurrence was completely random and all shocks were delivered, the failure to continue to meet the interresponse-time criterion insured that a more aversive shock was the next shock to occur. Accordingly, an averaging process based upon interresponse-time differences seems to be required.

Dinsmoor's (2001) characterization of molar avoidance theorists is not justified. Herrnstein and Hineline's (1966) assertion that "a response-dependent change in the amount of subsequent aversive stimulation appears to be the *sine qua non* of avoidance conditioning" (p. 429) established the authors as proponents of shock-frequency reduction as one form of reinforcement in avoidance rather than as the *only* form. Indeed, it was Hineline (1970), whose papers were singled out by Dinsmoor as exemplifying molar theory, who demonstrated that, when shock frequency is constant, shock delay is a powerful reinforcer of avoidance responses. In fact, so-called molar theory is better viewed as a preference for an operant account of avoidance on multiple temporal scales rather than as a one-factor account. A warning signal is treated as a discriminative stimulus occasioning the avoidance response, because that response has been negatively reinforced in the past by shock-frequency reduction, shock delay, or reduction in shock intensity or duration, or positively reinforced by proprioceptive or exteroceptive stimuli following the avoidance response. Although a conditioned aversive stimulus acquires its aversive properties through respondent conditioning and its termination functions as negative reinforcement, this by no means rules out the consideration of a warning signal as a discriminative stimulus. Thus, Dinsmoor's molecular rein-

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forcers are perfectly acceptable as reinforcers by so-called molar theory. In the case of free-operant avoidance, there is no compelling reason to treat the passage of time in the manner of a conditioned aversive temporal stimulus (Anger, 1963), rather than as a discriminative stimulus whose control over the avoidance operant increases with the passage of time, as in the temporal discrimination that develops with fixed-interval reinforcement. If the reinforcers, positive and negative, for avoidance conditioning identified by Dinsmoor are also reinforcers in the three-term contingency of stimulus control favored by Herrnstein (1969), it is difficult to see the relevance of Dinsmoor's strong rejection of shock-frequency reduction as the basis for the results in Sidman's (1962b) two-lever experiment or for the within-session variation of response rate in Sidman's (1962a) experiment in which each effective response resulted in 5 s of shock-free time to a maximum of 50 s.

Dinsmoor's (2001) critique of shock-frequency reduction is threefold. (a) Shock-frequency reduction is neither necessary nor sufficient for avoidance conditioning. Hineline (1970) showed that shock-frequency reduction is not necessary, and that shock delay is sufficient. Gardner and Lewis (1977), however, have provided evidence that shock delay is *not* necessary, and that shock-frequency reduction *is* sufficient. The fact that the imposed and alternative conditions in their experiments have different correlated exteroceptive stimuli is considered a confounding effect by Dinsmoor. The manner in which such a stimulus difference invalidates the finding that neither shock delay nor shock-frequency reduction is necessary, but that either is sufficient, requires clarification. Even if the stimulus change functions as a conditioned reinforcer (safety signal), its reinforcing properties must have been acquired as a result of the reduced aversiveness of the alternative condition. (b) Data interpreted in terms of shock-frequency reduction can be equally interpreted by two-factor theory, but not the reverse. Yet, if shock-frequency reduction without shock delay is sufficient, two-factor theory cannot always handle data that can be interpreted by treating shock-frequency reduction as an effective reinforcer for avoidance conditioning. (c) By its very nature, shock-frequency reduction cannot make contact with the individual response. This final criticism is based primarily on the fact that a reduction in shock frequency cannot be located in time. Once a shock has occurred, shock frequency decreases continuously with time. Therefore, such a decrease requires a change in the denominator of the fraction (shocks/time); in other words, an increase in the time unit. The same argument can be made against the use of response rate as a fundamental measure of operant strength. Like a shock, which does have a specific time reference and increases shock frequency in step fashion by one unit, a response increases rate in step fashion by one unit. Similarly, once a response has occurred, rate decreases in continuous fashion with the passage of time until the next response. In other words, rate is a molar construct in the same manner as shock-frequency reduction. Does that disqualify rate as a useful measure? Of course, it does not. The time unit is not changed, and a count is made of the number of responses occurring during that unit. Similarly, shock frequency is determined by counting the number of shocks occurring in an unchanged time unit. Of course, Dinsmoor can argue that the failure of shock frequency to make contact with the individual response disqualifies it as a reinforcer for avoidance conditioning. But how does a change in rate make contact with the individual response? The effect of such a change may be mediated by the differential reinforcement of interresponse times. But an increase in the interresponse time, like a decrease in response rate or a decrease in shock frequency, is continuous from the moment a response occurs and, by Dinsmoor's logic, has no location in time and, therefore, no contact with the individual response.

A few other comments follow: Dinsmoor (2001) poses the question of whether the termination of a warning signal and the production of a safety signal are distinguishable as reinforcement for avoidance responses. Even in the absence of a warning signal, the two-factor approach assumes that the change from the context plus the stimuli from ineffective responses prior to the avoidance response to the context plus avoidance-response-produced stimuli could be interpreted either as the equivalent of the termination of an exteroceptive warning signal or as the production of a safety signal. He cites Dinsmoor and Sears (1973) as providing evidence that a safety signal is distinguishable from the termination of a warning signal. However, in an analogous experiment in which a tone of a given frequency was reinforced by shock, the subsequent termination of that tone or tones of different frequency by the avoidance response, even in the absence of shock, would presumably result in a similar generalization gradient of reinforcing effectiveness, thus once more suggesting that termination of a warning signal and production of a safety signal are indistinguishable.

Lambert, Bersh, Hineline, and Smith (1973) found that, in a shuttlebox, a rat would press a lever or make a crossing response whose immediate consequence was a shock, provided that it prevented a delayed five-shock sequence. Dinsmoor (2001) must assume that, despite no immediate change in aversiveness from preresponse to postavoidance response, the overall reduction in aversiveness reinforces the response. Gardner and Lewis (1977) found that a pigeon would acquire a response that resulted in a change from an imposed to an alternative condition, even though there was no change in delay to the first two shocks in the alternative condition, provided that there was a 75% reduction in shock frequency. If Dinsmoor is to account for these results without accepting shock-frequency reduction as an effective reinforcer, he must now assume that a subsequent delay to the next shock (in the imposed condition) after no postresponse change in aversiveness for one or two shocks is sufficient. Gardner and Lewis failed to obtain stable responding when the alternative condition involved no change in delay to the first three shocks. The assumption presumably is now that this represents a limit to the effectiveness of a subsequent delay. But such an analysis would certainly be post hoc.

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